tive force, or tension, or stress, they are driven to thrust it into the body, notwithstanding the perplexities and contradictions caused by so doing, and notwithstanding the painful necessity incurred thereby of hoodwinking poor P. in the above manner, and endeavouring to hoodwink the metaphysicians. But there is really no reason why force with the saving reservation should not be introduced as freely into the discussion of E. as into other questions of dynamics; and the physicists often do introduce it thereinto; but then, when frightened at what they have done, they will silently withdraw it again. All the inconsistencies of the doctors, and their capriciously varying moods of freedom and shyness respecting "force," and their stepping up and down from one platform of thought to another, perplex poor P. beyond measure. He knows nothing but what they tell him; and he dares not attribute his difficulties to anything but either the abstruseness of the subject or his own stupidity.

But probably there was another motive, also, for this melancholy idea of putting the E. of position into the body, viz, the desire for simplicity of arrangement. Since E. is E. and the kinetic E. is undeniably in the body, it would seem to be an orderly proceeding to put the other there too. But this would be as if a methodical housekeeper should keep her coals and her blankets in the same "hole" because they are both warming apparatus, though in very different ways. And besides, we shall find that whatever may be the gain in this respect in putting both the types of E. into the body, it is outweighed by a certain loss of true correspondence and clear analogy, which will be mentioned farther on.

We now come to the more direct consideration of the merits of this procedure of putting the E. of position into the body. Let us begin with an interesting little illustration of its character. It is the ordinary and legitimate mode of expression to say that when a stone is projected vertically upwards, the gravitational attraction between the earth and stone draws the stone down again and gives it the kinetic E. with which it strikes the earth. And the gravitation attraction is usually and conveniently conceived and spoken of as being all the earth's: and the stone is usually regarded as being simply attract*ed*. Every doctor will frequently speak thus; and nevertheless he will also, and sometimes in the same breath,[1] tell us that it is the *stone*, say at the highest point of its ascent, that has E. of position due to its height from the ground. So then the connecting attractive force, which is to do the work of drawing the stone down again, and which is therefore one factor of the E. present, is regarded as being in the earth, but the E. as being in the stone! This is *one* way no doubt of teaching poor P. the difference between force and E! Take another illustration. Some of our foremost doctors[2] tell us that when a bow is drawn and about to discharge the arrow or the bolt it is the arrow or the bolt that has E. of position; in this they have at least the merit of consistency. Poor P. generally feels that this conveys no distinct idea at all to his mind; of course he dares not thing it wrong. Then he finds other doctors[3] who tell him (though in so doing they are inconsistent[4] with themselves) that in this case the E. is in the bow. What is to be done now? Is this distracting E. of position "like a bird so that it can be both here and there at the same time"? Or are the doctors on one side—how shall we write it—wrong? At any rate, since the doctors differ, poor P. must needs choose for himself, and in order to escape the above perplexities and also for the following reasons, he elects to conceive of the E. of position as not in the body but in the force or forces concerned which are at least virtually there; it being an ulterior and quite another question, what is force?

The discussion is of course now, as it has been all along, only as to modes of conception or of expression, and not as to the science of our doctors. All agree that if you *spend* E. against the resistance of the inertia of a mass in giving it velocity or acceleration, you have bestowed your E. on the inertia of that body, you have transferred your E. to that inertia. So, in exact correspondence and analogy, if you spend E. against the resistance of the gravitation attraction, for instance, in raising a stone to a certain height you have bestowed your E. on that attraction, you have transferred your E. to gravity. That attraction was beforehand pulling at the stone as hard as it could; but it had no power of doing work, according to the definition of work, *i.e.*, it had no energy according to the definition of E. You have given it E., or the power of performing work by affording it the condition necessary for its doing work, viz., space to work through. Why will not the doctors say this in so many words, when they do say it virtually in various forms? From Newton down they tell us this, that the work done by a force is fs (s being the space through which the force f acts); but the work done is the measure of the preceding E. or power which of course the force had of doing that work; why then will they scarcely ever say that the E. of a force is fs (s being now the space through which the force will have opportunity of acting)? When they do say in substance what we want them to say, they avoid most carefully the direct clear statement of it in so many words.[1] "This kind of E. [potential] depends upon the work which the forces of the system would do if the parts of the system were to yield to the action of those forces." That, of course, means *precisely the same* as the following, which, however, expresses the thing more directly. This kind of E. (potential) is the E. which the forces of the system possess in consequence of the possible displacements of the parts of the system under the action of those forces. Tait himself, both in his Glasgow lecture and in his "Recent Advances," tells us that a wound up spring or bent bow has potential E. Clerk Maxwell tells us the same. If so we have a right to speak of the energy of the gravitation attraction. In a certain respect the cases are different, but not so as to affect the present point.

This, our putting of the E. of position into the forces, instead of into the body or bodies, does not, of course, *explain* the action any more than the other does, but it gives a conception (provisional, if you like) which is much clearer and *in better analogy*, and, as we have said, free from all the above-recounted confusions. Moreover, the expression "E. of a force" has the great advantage of keeping before the mind of poor P. the fact that force and energy are not the same, a distinction which he is slow to apprehend, and which it is of the utmost importance to him that he should get proper hold of.

And now that we have got our E. of position into its most convenient seat, what shall we call it, and how shall we speak of its action? We cannot be dreadfully wrong if we call it by a name suggested by an expression of Helmholtz; let it be "*Energy of Tension.*" Does it not seem more logical to designate it by its essential characteristic than by what is only a condition though an indispensable one; for this latter we do when we call it E. of position or configuration. And as to its action let us say that when E. is being, as it is usually expressed, transformed from potential to actual E., or *vice versâ*, it is *transferred* from the forces to the bodies of the system, or *vice versâ*. If these expressions are unsuitable and erroneous, then let every one abstain from language which is precisely tantamount to them. But our doctors do not do this; and it fortifies us greatly in the belief that we are right to know that our doctors, when they are quite themselves, say the very same in substance, though not in so many words. On the other hand, if these expressions recommend themselves to us, let us use them boldly and consistently without mincing matters. Deschanel seems to have been on the point of using them in one place.[2] However, the fear of his *confrères* suddenly rose before his eyes, and having written (or his translator for him) the word "transferred," he stops short without telling us from what and to what the transference is made; he leaves us to complete for ourselves the sense of the passage, which clearly is that the transference is from the forces to the bodies, and *vice versâ*.

Poor Publius and myself have *several* other complaints to make; but probably we have said enough to excite the sympathy of all considerate persons.

Dublin X.

[1] Clerk Maxwell's "Heat," p. 281 bottom; see Willson's "Dynamics," p. 147.

[2] *E.g.* Balfour Stewart, "Cons. of E.," p. 25 (but see his "Elem. Phys.," p. 106).

[3] Tait, "Recent Advances," p. 18; Willson, "Dynamics," p. 278.

[4] The inconsistency is startlingly exhibited in a single sentence for which two doctors are responsible, "Uns. Univ.," p. 111, "the potential E. of a raised weight or bent spring." If the potential E. is in either one of these it cannot be in the other. We have the same in a single sentence in Thomson and Tait, p. 178 (two doctors, again, responsible); also in Tait's Glasgow lecture.

[1] The only exception that I remember to have seen is afforded, curiously enough, by Rankine himself, the inventor of E. *in posse.* In *Phil. Mag.*, February, 1853, he says, "E. of gravitation;" and in "Encycl. Brit.," vol. xiv., "Mechanics," he speaks of the E. of an effort.

[2] "Nat. Phil.," p. 79

New Electric Lights

UNDER the above title Mr. Munro describes, in NATURE, vol. xvi. p. 422, M. Lodighin's device for an electric light. This is no novelty but a simple repetition of an invention made

by Mr. Starr, a young American, and patented in this country under the title of "King's Patent Electric Light," specification enrolled March 25, 1846. An account of it, with drawings, may be found in the *Mechanic's Magazine*, April 25, 1846, p. 312. To this are appended some editorial remarks in which the novelty of the invention was at that date disputed. Those who care to follow the subject further may find a letter of mine replying to this editorial criticism in the *Mechanic's Magazine* of May 9, 1846, p. 348.

I constructed a large battery and otherwise assisted Mr. Starr in his experiments on this light. The "wick," as Mr. Munro aptly calls it, was a stick of gas retort carbon, like that pictured (NATURE, p. 423), excepting that it was affixed to supports of porcelain in order to remedy the fracture which occurred to our first apparatus in which the carbon stick was rigidly held in metallic forceps. Thus the improvement of M. Kosloff was also anticipated.

The lamp-glass was a thick barometer tube about thirty-six inches long, with its upper end blown out to form a large bulb or expanded chamber. The carbon and its connections were mounted in this with a platinum wire passing through and sealed into the upper closed and expanded end of the tube.

The whole of the tube was then filled with mercury and inverted in a reservoir, and thus the carbon stick, &c., were left in a Torricellian vacuum. The current was passed by connecting the electrodes of the battery with the mercury (into which a wire from the lower end of the carbon dipped) and with the upper platinum wire respectively. A beautiful steady light was produced accompanied with a very curious result which at the time we could not explain, viz., a fall of the mercury to about half its barometrical height and the formation within the tube of an atmosphere containing carbonic acid.

I have now little doubt that this was due to the combustion of some of the carbon by means of the oxygen occluded within itself.

In pointing out this anticipation of M. Lodighin's invention I do not assume or suppose that any piracy has been perpetrated. It is one of those repetitions of the same idea which are of such common occurrence and which cost the re-inventor and his friends a vast amount of trouble and expense that might be saved if they knew what had been done before.

I may add that the result of our battery experiments was to convince Mr. Starr that a magneto-electric arrangement should be used as the source of power in electric illumination; and that he died suddenly in Birmingham in 1846, while constructing a magnetic battery with a new armature which, theoretically, appeared a great improvement on those used at that date. Of its practical merits I am unable to speak.

Twickenham, September 18 W. MATTIEU WILLIAMS

Serpula Parallela

Two or three years ago I read somewhere that *Serpula parallela* of M'Coy is probably a vitreous sponge. Can any of your readers give me a reference for this? I wish to give the authority for this happy suggestion to which Mr. Young and I referred last year. JOHN YOUNG

Glasgow University, September 19

HYDROGRAPHIC SURVEY OF THE BALTIC

WE learn from the Stockholm *Nya Dagligt Allehanda* that during the month of July last a hydrographical survey of the Baltic was carried out by two vessels belonging to the Swedish navy, which were placed for this purpose at the disposal of the Swedish Royal Academy of Sciences for a month. A grant of about 550*l.* is intended to cover the expenses of three such expeditions. The whole of the Baltic, from a line drawn from Arendal to Jutland to the head of the Gulf of Bothnia and from the Swedish coast on the one side to the Finnish, Russian, German, Danish, and Norwegian on the other, was examined for temperature and salinity along thirty-four lines, measuring together more than 23,000 English miles, and including 200 stations. At every such station the temperature and salinity of the sea water were ascertained at the surface and at several different depths down to the bottom, about 1,800 different determinations of temperature having been made and a corresponding number of samples of water obtained. The nature of the bottom has also been ascertained by instruments which brought up samples not only from the surface of the bottom, but also from a variable depth, occasionally several feet, under it. The plan of this survey, which is said to be the most complete that has yet been made for its special objects, the determination of the salinity and temperature, was drawn up and carried out by Prof. F. L. Ekman. New instruments for taking samples of sea-water at different depths were employed, and as the temperature of the water did not undergo any perceptible alteration during the time required for getting it to the surface, for every sample that was obtained, the temperature of the depth from which it was raised was ascertained simultaneously, without any great loss of time. The survey shows the Baltic and the Gulf of Bothnia to consist of three strata, differing greatly in temperature, and often very sharply defined, viz., an upper stratum, which is warmed during the summer by the heat of the sun to a pretty high temperature, a lower, in which the cold of winter still prevailed to a great extent, and under the latter still another of a somewhat higher temperature than the intermediate stratum, the third stratum being of great thickness where the depth was considerable. In the Gulf of Bothnia, as in Skagerack and Kattegat, on the other hand, the temperature diminished steadily in proportion to the depth, as is commonly the case in the ocean. The uppermost summer-warm stratum of water was found to be of variable thickness at different places in the Baltic; at some it was scarcely perceptible at the period of observation. This and other peculiarities will probably be explained in the course of the working out of the observations which is now proceeding.

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—The following ephemeris of this satellite for the next period of absence of moonlight is founded upon the elements calculated by Prof. Asaph Hall, of Washington, from his measures in 1875. Though limited to dates when Saturn may be observed while the moon is absent, probably her presence, except when very near the planet, is less an impediment to viewing so faint an object than the unavoidable proximity of the planet itself.

At 10h. Greenwich M.T.

		Pos.	Dist.			Pos.	Dist.
		°	"			°	"
Sept.	30	261·8	47·8	Oct.	6	277·4	219·5
Oct.	1	270·6	118·6	,,	7	278·8	177·4
,,	2	273·0	176·6	,,	8	281·2	130·8
,,	3	274·3	215·8	,,	9	286·7	76·5
,,	4	275·3	234·1	,,	10	321·0	22·1
,,	5	276·3	232·3				

An ephemeris of the five inner satellites of Saturn, by Mr. Marth, appears in No. 2,154 of the *Astronomische Nachrichten*. It is elaborately compiled, but this the first portion, extending to September 20, only reached this country on the date of its expiration. It is to be regretted that a work of this interest involving so much care and trouble in its preparation, should not have been in the hands of astronomers earlier; it is not the first instance of unfortunate delay in the publication of communications of immediate utility in this periodical of late.

THE NEW COMET (1877, IV.).—A first approximation to the orbit of the faint comet discovered at Marseilles on the 14th inst. calculated by Mr. Hind upon M. Coggia's observation on that date, and observations at Leipsic by